

Contemporary Psychology

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- Page 81 **New Directions in Psychoanalysis**, by Melanie Klein, Paula Heimann, and Roger Money-Kyrle (Eds.)
The Psychoanalytic Study of the Child, by Ruth S. Eissler, Anna Freud, Heinz Hartmann, and Ernst Kris (Eds.)
Reviewed by R. W. WHITE
- 85 **Materialy Soveshchaniya po Psikhologii**, by B. G. Anan'yev *et al.*
Reviewed by GREGORY RAZBAN
- 87 **The Organization Man**, by W. H. Whyte
Administrative Behavior: A Study of Decision-Making Processes in Administrative Organization, by H. A. Simon
Reviewed by E. F. BORGATTA
- 90 **Evaluating Student Progress in the Secondary School**, by Alfred Schwartz and S. C. Tiedeman
Reviewed by R. T. LENNON
- 91 **Decision Making: An Experimental Approach**, by Donald Davidson and Patrick Suppes, with Sidney Siegel
Reviewed by EUGENE GALANTER

(Continued on inside cover)

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CONTENTS—continued

93 CP SPEAKS

By the EDITOR

98 Culture and Mental Disorders, by Ralph Linton

Reviewed by M. K. OPLER

100 Introductory Clinical Psychology: An Overview of the Functions, Methods, and Problems of Contemporary Clinical Psychology, by S. L. Garfield

Reviewed by R. L. CUTLER

102 Dance in Psychotherapy, by Elizabeth Rosen

Reviewed by R. W. PUGH

104 Neuropsychopharmacology, by H. A. Abramson (Ed.)

Reviewed by SHERMAN ROSS

105 The Person in Psychology: Reality or Abstraction, by Paul LaFitte

Reviewed by G. W. ALLPORT

106 Experimental Psychopathology, by P. H. Hoch and Joseph Zubin (Eds.)

Reviewed by A. H. BLACK

109 FILMS

ADOLPH MANOIL, Film Editor

Mental Deficiency, review by G. M. GUTHRIE

Perception, review by H. C. MAHAN

110 List of Books Received

Headpieces for CP SPEAKS, FILMS, and ON THE OTHER
HAND, by ROBERT M. LOVELL

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Contemporary Psychology

A JOURNAL OF REVIEWS

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Psychoanalysis and Children: A House Still Divided

Melanie Klein, Paula Heimann, and Roger Money-Kyrle (Eds.)

New Directions in Psychoanalysis. New York: Basic Books, 1956. Pp. xiv + 534. \$7.50.

Ruth S. Eissler, Anna Freud, Heinz Hartmann, and Ernst Kris (Eds.)

The Psychoanalytic Study of the Child. Vol. XI. New York: International Universities Press, 1956. Pp. 470. \$8.50.

Reviewed by ROBERT W. WHITE

Dr. White, long of Harvard Psychological Clinic and now chairman of Harvard's Department of Social Relations, now reaching adolescence in its eleventh year, is one of CP's consultants and staunch reviewers. He reviewed the second volume of Ernest Jones's *Freud in February 1956* (CP, 1, 35-40) and the *Ausbachers'* Alfred Adler in *January 1957* (CP, 2, 1-4).

WHEN news began to reach America, more than thirty years ago, that psychoanalytic methods were being used with children, there were already two well-marked schools of thought on the subject. One school, led by Melanie Klein at Berlin, claimed credit for inventing the play technique, which served as a substitute for adult free associations and thus permitted psychoanalysis to proceed with young children exactly as it did with grown-up patients. The other school, given voice

by Anna Freud at Vienna, seemed more concerned about the differences between children and adults, and proposed several changes in the usual technique when the patients were of younger years. Paradoxically, Freud's written views on therapeutic technique were faithfully reproduced in Mrs. Klein's work, while his daughter, always working closely with him, introduced novelties which in those days might almost have been called 'neo-Freudian.'

Melanie Klein began her work in the early twenties, but her views became widely known here only in 1932 with the publication of *The Psychoanalysis of Children*. The most striking feature of her method, apart from the play technique which has since won general acceptance, was the early and consistent use of interpretation, exactly as might be done with adult free associations or dreams. Most readers of this first book were startled by the speed with which

Mrs. Klein perceived sexual meanings in the child's handling of toys at the first interview, and by her prompt communication of these thoughts to her youthful patients. Mrs. Klein maintained, however, that in this way she created the only proper climate for psychoanalysis, and that in consequence there developed a true transference—a re-animation in analysis of the root difficulties with the parents—and an ultimate cure in the sense of bringing to consciousness the important things that had been repressed.

Anna Freud, who turned to psychoanalysis after some years as a teacher, began her work also in the early twenties. Her monograph entitled *Introduction to the Technique of Child Analysis*, which appeared in English in 1928, appealed to many readers for its clarity and common sense. Miss Freud doubted that the therapeutic situation could have the same significance for children, brought by their parents, as it had for adults aware of their own needs. The conditions necessary for analytic treatment could be created only after a preliminary period during which the therapist made herself the child's useful friend. She also questioned the similarity of the transference in a situation where the parents were still the major force in the child's life, and she thought it necessary to work with the parents in order to achieve a lasting improvement in the child. All these innovations were rejected by Mrs. Klein, who considered them foreign to the spirit of Freudian psychoanalysis.

Both leaders eventually moved to England, but Mrs. Klein arrived first

and soon gathered around her a considerable group, including both refugees and English analysts, which presently became known as the "English School." It was soon apparent that her original fidelity to Freud—or perhaps it was to Karl Abraham, her analyst and teacher—would not stand in the way of rather striking changes in psychoanalytic theory. Working frequently with "pre-oedipal" children, she became convinced that the superego was by no means the product of the Oedipus situation but had its origin much earlier in life. Pursuing this theme, she gradually pressed back into the first year of life many of the significant events, including the Oedipus complex itself, that Freud had seen as spreading through five years, and she was thus forced to reconsider several fundamental aspects of psychoanalytic theory. Her ideas, which seemed to flourish on English soil, made but a slight impression in the United States, where there has been little serious attempt to weigh their merits.

Anna Freud in the meantime stayed with her father through his belated migration to England in 1938 and his death in 1939. During World War II she established the Hampstead Nursery in London as a haven for young children, and her subsequent publications made a substantial contribution toward understanding the origins of neurosis and the developmental consequences of parent-child relationships. Her work has been closely and eagerly followed in the United States, where it is quite the predominant influence among workers with children who consider themselves psychoanalytically oriented.

Two books are at hand which provide us with the opportunity to bring this history of the schools of child psychoanalysis up to date. One of these, *New Directions in Psychoanalysis*, is frankly a tribute to Mrs. Klein, being assembled largely from papers presented on the occasion of her seventieth birthday. This circumstance no doubt gives it an unduly provincial atmosphere, but we can properly use it as an expression of Mrs. Klein's theories in their full development. The other book, the eleventh volume of *The Psychoanalytic Study of*



MELANIE KLEIN

the Child, does not have the same air of finality or even of agreement, but we can derive from it an idea of the current state of affairs among child analysts partial to the views of Anna Freud. Have the two schools diverged more widely in the course of thirty years, or have they drawn closer together? And how do they stand with reference to their common point of origin, the observations and theories of Sigmund Freud?

One trend can be detected which is common to both books. This is a decline of interest in neurosis and a corresponding rise of fascination by the problems of psychosis. Most of the patients who appear in *New Directions* are schizophrenics, either child or adult; most of the children reported in the other book show some form of early "ego defect" or "ego disturbance." Both schools have moved their interest to the earliest stages of development, to those processes which constitute first steps in the development of the ego. In thus shifting their emphasis from therapeutic concerns to fundamental investigation they have ceased to be schools of child analysis as such. We are dealing here with two versions of general psychoanalytic theory.

It is made quite clear by *New Directions in Psychoanalysis* that Melanie Klein's group has evolved into a sharply distinctive branch. This can already be

seen in Mrs. Klein's opening paper, which describes the historical course of her several discoveries, and in Paula Heimann's re-evaluation of the Oedipus complex and the libidinal stages of development. In certain respects these two important papers affirm their loyalty to Freud: the death instinct, rejected by most analysts, is fully accepted by the English school, the stages of psychosexual development are retained, and the new picture of early infancy "makes us realize all the more the truth of Freud's discovery that the Oedipus complex is the nuclear complex in the individual's life" (Heimann). Yet the remaining papers in the first part of the book, partly theoretical but chiefly clinical, keep Mrs. Klein's ideas very much in the foreground, and their employment seems to obviate to a surprising extent any need for the more familiar developmental concepts of psychoanalysis.

One of her most characteristic ideas has to do with object relations. "In tracing, in the analyses of adults and children," she says, "the development of impulses, phantasies, and anxieties back to their origin, i.e., to the feelings towards the mother's breast (even with children who have not been breast-fed), I found that object relations start almost at birth and arise with the first feeding experience; furthermore, that all aspects of mental life are bound up with object relations. It also emerged that the child's experience of the external world, which very soon includes his ambivalent relation to his father and to other members of his family, is constantly influenced by—and in turn influences—the internal world he is building up."

The instincts, she maintains, give rise to unconscious fantasies which are not the product of experience, though they are quickly influenced by it. Thus the infant's desire for the mother's breast produces the fantasy of incorporating it, and this is speedily amplified by the sensations of sucking and swallowing, which in their turn are interpreted in accordance with the fantasy. In this way Mrs. Klein sets up a dialectic of inner and outer objects; in the earliest months the two realms frequently exchange contents through the mechanisms of introjection and projection. The good

breast, yielding satisfaction, becomes firmly established in favorable cases as an internal object, serving as "a focal point in the ego" and a foundation for psychological health. But there is also the bad breast which denies satisfaction and evokes envious aggression; this has to be projected into the external world. The whole process soon grows more complicated, but throughout the book the fate of the child's objects, good and bad, internal and external, becomes a new language for describing his mental state and psychodynamic problems. So pervasive are these concepts that we are almost justified in speaking of Mrs. Klein's 'objectology.'

Closely related to these ideas are her views on the major emotional problems of the early months. Up to the fourth or fifth month, the main source of anxiety is the child's own destructiveness, which because of the mechanism of splitting and the projection of bad objects is felt as a persecuting external force. The infant is thus in the "paranoid-schizoid position," and his experiences at this time, if unfavorable, lay the foundation for schizophrenic psychosis. By the middle of the first year the ego has gained the strength to integrate good and bad objects, so that the good and bad mother, for instance, can be apprehended as one person. Anxiety is now aroused by the danger that aggression will injure or destroy the mother, and the infant enters the "depressive position," the seed-bed of manic-depressive psychosis. Here he may regress to the paranoid-schizoid position, deny the anxiety by a manic reaction, or master the difficulty by developing means of making reparation to the mother, a course which is highly favorable for future development. It is made clear in Heimann's first essay (Chap. 2) that Abraham's and Freud's psychosexual stages are by no means rejected, but, if we take *New Directions* as a whole, they very much fade out in favor of the two "positions" and their consequences.

It is unfortunate that Mrs. Klein and her followers constantly refer to these ideas as her "discoveries." Obviously she has not analyzed children during the first year of life. What is presented is, therefore, a hypothetical reconstruc-

tion based upon material gathered at later ages, and this entails the risk that intervening mental development may have contaminated the experiences of the first year. I have come to have a great respect for Mrs. Klein's persistent attempts to feel her way into the infant's mind, but I see no way of being certain that she has placed the right things at the right times. Conviction is not strengthened by the disquieting insularity of the English School which, judging by the references, feels little need to learn from outsiders such as Piaget, Anna Freud, or Hartmann. And the second part of the book, which presumably aims to fortify the argument by showing how well Mrs. Klein's concepts explain literary, artistic, and sociological materials, seems to me rather to weaken the case. Too much is too easily explained.

THE difficulty about being certain is of course a universal problem for psychoanalysis, and we do not find it resolved when we turn to *The Psychoanalytic Study of the Child*. This annual, managed by Ruth S. Eissler, Anna Freud, Heinz Hartmann, and the late Ernst Kris, has come to occupy an influential position in the American literature on psychoanalysis. A glance at the reference lists in this eleventh volume suggests that this group of workers is less insular than the English School.



ANNA FREUD

To be sure, the references to "This Annual" are rather overwhelming, but Piaget and other developmental psychologists as well as Mrs. Klein and her followers are given an occasional hearing, and there is one paper which actually reports an experiment. When it comes to style, the differences observed between Anna Freud's first monograph and Melanie Klein's first book seem to be repeated in the two books written largely by their followers. Several contributors to the annual do not share Miss Freud's gift of clarity, revealed anew in her perceptive paper in this volume, but the average level of obscurity is certainly lower than in *New Directions*.

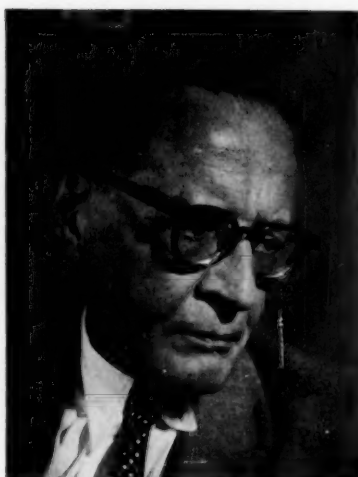
Those who have perused this annual over the years will be aware of the gradual emergence of Heinz Hartmann as its chief spokesman on matters of theory. His papers, either alone or with Ernst Kris and Rudolph Loewenstein, constitute a searching re-examination of Freudian theory, especially in its developmental aspects. The extent of his influence can be gauged by a change in the typical format of papers written by others: the usual initial references to the weighty words of Freud himself are now apt to be followed by similar citations from Hartmann before the author proceeds to his own contribution. In this eleventh volume Hartmann has a paper entitled *Notes on the Reality Principle*, which seems to me the clearest expression he has yet given both of his own position and of its relation to Freud. Its theme is broad enough to indicate his main new directions and thus to afford comparison with Mrs. Klein's new directions.

Hartmann believes that ego functions enter into the child's growth as an independent variable in addition to need gratifications, frustrations, and anxieties. The reality principle cannot arise simply from frustration; it involves postponement and anticipation, and it presupposes the existence of these capacities at least in elementary form. This suggests that "the activities of the functions that constitute the reality principle can be pleasurable in themselves," that the infant has autonomous positive interests in object relations, and that pleasure cannot be correlated with tension reduc-

tion. Children take pride in foregoing a pleasure and in being allowed to participate in the adult world; they are interested in reality as well as in gratification. Their behavior demonstrates not only the familiar unconscious defense mechanisms but also many "defense maneuvers" which may be conscious or preconscious.

All of this does not sound much like Freud, as most people think of him, but Hartmann is generally able to support his innovations with recent statements by Anna Freud and with direct quotations from Freud's later writings (1924-1939). It is true that the latter are sometimes single remarks casually dropped, but we learn from them that Freud came to doubt several of his earlier views and opened his mind more fully to the problems of ego psychology. Hartmann would like us to believe that his own work and that of Anna Freud is only what Freud would have done if health and strength had permitted. Perhaps he is right.

WHEN we try to compare the theoretical outlook set by Hartmann with that of Mrs. Klein, it seems at first as if they were miles apart. Yet the starting point is the same: the papers in both volumes are very much concerned with the first year, the earliest steps of development, and the problems of psychosis. It seems to me that the major difference lies in what the two schools consider to be most important. Mrs. Klein's group is still concentrating on uncovering the primitive irrationalities that beset our lives. When a piece of behavior is shown to be a re-animation of the depressive position, for instance, then we understand it. Only in her doctrine of reparation is there a hint that human beings can do more than be lived by their lives, as Charles Berg has so happily expressed it. This is in the classic psychoanalytic tradition, the one which Freud was slowly beginning to abandon at about the time when Mrs. Klein's work began. The group associated with *The Psychoanalytic Study of the Child* has begun to take seriously the challenge of ego psychology and also of individual differences. Some contributors, to be sure, are still fascinated



HEINZ HARTMANN

by the search for the root complex, but others deal with differences in drive endowments and sensitivities, and still others work at the problem of ego defects as cumulative events in development. This group seems headed toward making psychoanalysis a psychology of personality rather than a mere psychology of the unconscious.

The theme of differential sensitivity is examined by Lustman, who measured the autonomic responses of 4-day-old neonates to stimulation of the lips, skin, anus, and genitals. On the average the lips are most sensitive, but there are also great individual differences in pattern and in total reactivity which can easily be seen as affecting the feeding experience and the mother's response. The same theme is pursued further in a joint paper by Alpert, Neubauer, and Weil, who present three cases with different drive endowments (different patterns of high and low libidinal and aggressive energies) and work out in detail

the interaction of endowment and environment in the course of ego development. A long paper on schizophrenia by Beres is particularly useful for its searching review of the psychoanalytic conception of ego functions. Other papers, for example those by Boyer and by Pavenstedt, give detailed pictures of the cumulative effect of somewhat pathological maternal behavior upon the development of object relations, spontaneous behavior, and the processes of control and defense. Of course there is plenty of speculative reconstruction in all this, but in comparison with *New Directions* there is a much wider range of observation and a richer supply of concepts.

Thus it appears that the two schools of thinking and speaking, which began in the early twenties, have not drawn closer in the ensuing years. Their interpretations of early development differ on fundamental points, notably in respect of the innate unconscious fantasies, the timetable of mental development, and the position occupied by projection and introjection. *The Psychoanalytic Study of the Child* contains a paper by Elizabeth Zetzel which points out these basic divergencies but at the same time reminds us that Melanie Klein led the way with certain ideas now generally accepted by analysts as a whole. Dr. Zetzel would award pioneering honors to Mrs. Klein for her vision of the importance of anxiety, aggression, object relations, interaction with the mother, and depressive and reparative tendencies. With somewhat more certainty we can add her development of the play technique. Nevertheless it is clear that these two schools of psychoanalysis are actually cousins under the skin, and that can be true no matter how little each speaks the other's language.



They may abuse my theories by day, but I am sure they dream of them by night.

—SIGMUND FREUD



Flash! Psychology Inside the U.S.S.R.

B. G. Anan'yev et al. (Eds.)

Materialy Soveshchaniya po Psikhologii (Materials of the Conference on Psychology). Moscow: Akademiya Pedagogicheskikh Nauk RSFSR, 1957. Pp. 732. 26.74 rb. (2000 copies.)

Reviewed by GREGORY RAZRAN

Dr. Razran, whom CP has already introduced as one of America's principal afferent channels for news about psychology in the U.S.S.R., has recently reviewed the current Russian textbooks in psychology (CP, Apr. 1957, 2, 93-101). Now he comes across with the Russians' own latest summary of their current research. In these reviews CP departs from its rule of printing only critical comment, letting its readers go to the books themselves for the detailed facts, for the reason that Russian is still much less accessible to English-speaking readers than French and German—the books are obtained with difficulty, and the ideas are, too, after you have the book. Dr. Razran is Professor of Psychology at Queens College in Flushing, New York, and Chairman of its Department of Psychology.

HERE we have what is by far the most massive and the most instructive and representative single book of present-day Soviet psychology. It consists of reports—most likely, full reports—of 97 papers delivered at the Third All-Union Conference on Psychology held in Moscow, 1-6 July 1955. At the preceding All-Union Conference in 1953 (there was no such conference in 1954), the conference mentioned in an earlier review (CP, Apr. 1957, 2, 99-101), the number of papers read was 31 and they totaled 348 printed pages. The earlier review contained a statement that "there is no doubt that since about 1953 Soviet psychology has been proliferating," which is just about what it seems to be doing.

Besides an introductory paper by

Smirnov on present status and "what is to be done," and a historical paper by Rubinstein on Sechenov (on the occasion of the 50th anniversary of his death), the papers are grouped into twelve divisions, corresponding presumably in some way to sessions held. The divisions are named:

1. *The Psychology of Personality. The Psychology of Education.* 10 papers.
2. *Physiological Foundations of Man's Psychical Activities.* 7 papers.
3. *Sensations and Perceptions.* 16 papers.
4. *Perception and Understanding of Speech. The Development of Speech.* 7 papers.
5. *Speech and Action. Speech Mechanisms.* 6 papers.
6. *Intellectual Development of the Pre-School Child.* 4 papers.
7. *Cognitive Processes of the School Child.* 7 papers.
8. *The Acquisition of Spatial Imagery and of Technical Skills by School Children.* 7 papers.
9. *Problems in the Psychology of Work and Sport. Motor Habits.* 13 papers.
10. *Problems of Mental Sets.* 4 papers.
11. *Problems of Psychopathology and Defectology.* 9 papers.
12. *The Study of Animal Behavior.* 5 papers.

The mean size of the papers is 7.6 pages, about 4,200 words, and the three longest papers are 21, 16, and 14 pages. Presumably in this respect the Soviet psychologists are less restricted than are the Americans in their papers delivered at annual conferences.

THE divisions obviously silhouette—though only roughly—fields of interest.

It is clear, for instance, that Soviet psychologists are at present engaged but little in studies of animal behavior. Indeed, of the five papers in that division, one is by a physiologist, another by an animal trainer, and the third is a discussion—or rather, an exposition—of Pavlov's views on the ape experiments performed in his laboratory in the last years of his life. (Pavlov planned to address the 11th International Congress of Psychology on the subject.) It is not, of course, that animal studies are neglected or underdeveloped in the Soviet Union or that Soviet psychologists do not attach sufficient importance to them, but merely that such studies are as a rule referred to another group of professionals, the physiologists. The two experimental papers by psychologists both pertain to ape psychology, one being by Ladygina-Kohts, whose early pioneer studies of Ioni, the chimpanzee, have been well publicized outside Russia by Yerkes' *Great Apes*.

Another area in which present-day Soviet psychology seems to be little involved—and that may surprise some readers—is mental health. The Division of Psychopathology and Defectology contains only nine papers, and of these, six are primarily theoretical—reflex nature of hallucinations, perceptual and imaginal processes in the blind and the deaf-mute, cognitive speech and aphasia, and types of thought disturbances—whereas only three papers fall at all within the bailiwick of psychology's wonted clinicism. One reason for this state of affairs—so different, for instance, from the situation in the United States—is no doubt that, just as animal behavior is in the Soviet Union largely deferred to physiologists, clinical practice is deferred to psychiatrists. However, other reasons also exist, such as (a) the belief that the educational philosophy and praxis of Communism are in themselves psychologically prophylactic and rehabilitative, and (b) the ban on Freud and his orthodox and even unorthodox followers, and consequent restrictions upon the awareness of mental problems. Freud is not mentioned once in the entire book. Pavlov is quoted more or less extensively in 81 of the 97 papers, being cited in many of the papers more frequently than the

findings and reasonings of the authors themselves; but the quotations from Marx, Engels, Lenin, and Stalin are, contrary to earlier customs, not very numerous,—just occasional. Some change seems to be taking place.

On the other hand, it may be seen that Soviet psychologists are much concerned with problems of sensation and perception, speech and language, and higher mental processes. Language, in particular, under the name of the "second signal system," appears to be a dominant theme, for papers pertaining to it appear in 11 of the 12 Divisions. Again, one may discern a strong interest in the applied-educational phases of psychology. The significance of bracketing the *Psychology of Personality* with the *Psychology of Education* in one division lies in the fact that the papers in that division deal largely, not with general psychological problems and formulations of personality, but with concrete educational means of instilling in children desirable personality traits—discipline, responsibility, loyalty, social consciousness, morality, interests, and the like. (The concern with interests is the closest that Soviet psychologists come to the study of motivation, which otherwise is seldom treated as an experimental category.) Likewise, most of the experiments in children's thinking, imagination, concept formation, and skills seem to have been designed with teaching objectives in mind. As indicated in my earlier review, the applied and the academic are often so tightly interwoven in Soviet psychology that a good deal of its research is a kind of educational action research. It is, however, an action research that is primarily laboratory-based, and Soviet psychology in general is predominantly an experimental psychology, though, to be sure, experimental only in so far as its findings do not conflict with the basic tenets of the Soviet system.

PASSING now from mere *Beschreibung* of categories to *Kundgabe* of contents, I regret that space permits the consideration of only two areas in which, in my opinion, post-1950 Pavlovianized Soviet psychology has shown the most



KONSTANTIN NIKOLAYEVICH KORNILOV

Born in Siberia, 9 March 1879. Died in Moscow, 10 July 1957. Outstanding theoretical and experimental psychologist, founder of Reactology, attempted to synthesize psychology with Marxism-Leninism, retained influence although officially repudiated in 1931 as insufficiently Marxist-Leninist.

advance and novelty. These areas are Perception and Verbal Conditioning.

Post-1950 studies of perception began with the view that its "material" basis is no more than Pavlov's investigatory, orientating, or "what-is-it" reflex. We must remember that to Pavlov (and to Sechenov) the reflex is a ubiquitous concept comprising not only peripheral action of single effectors but also (and more so) centrally-cored total activities, that are—and they always are—neurally guided and deterministically correlated with physical input. That relation holds even if they are only verbal reports of subjective experiences. Hence, Soviet studies of perception have become complex undertakings. Simple answers, like "I see a dog" or "I hear a bell" or "I touch a ball," are swamped under an array of recordings of almost the body's entire repertoire, with special emphasis upon exact measurements of eye and head movements, and of vasomotor, respiratory, digestive, pupillary, eyelid, electrodermal, and electroencephalographic reactions. Moreover, in parallel with conditioning experiments,

thorough investigations are made of the waxing and waning of the "material bases of perception" in interaction with such variables as repetition, administration of drugs and chemicals, verbal instructions, temporal variations, extra and related stimuli, special physiological and psychological states. Needless to say, the results are interpreted in terms of the familiar Pavlovian concepts of extinction, spontaneous recovery, inhibition, disinhibition, irradiation, differentiation, induction, and the like. Pavlovian psychophysiology, it is maintained, is an all-reflex or all-behavior psychophysiology. It is not just one of conditioned reflexes or conditioned behavior.

IN 1933, Pavlov said that "monkeys [unlike dogs] have highly developed investigatory reflexes" and that, "wholly irrespective of food reactions, monkeys are continually moved to investigate" (*Wednesday Seminars*, vol. 2, 68–69; cf. Harlow's *manipulation motives*). This statement has prompted Soviet psychologists to ponder on the evolution of the investigatory reflex and to declare that the reflex is particularly dominant in man and, differently viewed, corresponds to what is commonly known as curiosity (cf. Harlow again). Now Sokolov, in one of the papers in the present volume, postulates, on the basis of an apparently carefully controlled experiment, that perception involves two separate sets of reflexes, *investigatory* and *adaptive*, differing from each other radically in a number of functional characteristics. Investigatory reflexes manifest, for instance, marked extinction and disinhibition, but correlate only generally with stimulus parameters, whereas the adaptive reflexes correlate very specifically with stimulus attributes but show little extinction and disinhibition. There is no doubt that Soviet physicalistic studies of perception are highly significant, not only for their own worth, but also as counterweights—or complements—to phenomenalist approaches to the topic, on the one hand, and to peripheralists' tendencies to ignore the topic altogether, on the other.

RUSSIAN studies of verbal conditioning—that is to say, reflexes conditioned

to verbal stimuli—began early but did not gain significant momentum and scope until very recently. Let me cite reports of two papers delivered at the Conference. Markosyan conditioned the blood coagulation of human subjects to the sound of a metronome and to the flash of an electric lamp, and he found that the conditioning could be transferred to the sound of the words *metronome* and *lamp*, as well as to the sound of the phonetically related words *metrostroy*, *metropol'*, *microtome*, *microscope*, and *meter*, and the sound of the semantically related words *lantern* and *light*. Elkin conditioned the eyelid reflex of 25 school children to a number of sentences, including *Segodnya solnechny den'* ("it is a sunny day today") and *Idyot dozhd'* ("it is raining"), and found that the conditioning was very easy when the to-be-conditioned sentence corresponded to the prevailing weather but very difficult, or impossible, when the weather during the conditioning and testing was different from that given in the sentence. (In an experiment by Volkova, not reported at the Conference, it was discovered that the salivary reflexes of school children who had been conditioned positively to the word *right* and negatively to the word *wrong* carried over correctly to right and wrong statements; e.g., to "8 divided by 2 is 4" vs. "10 divided by 2 is 3" and to "Snow melts in Spring" vs. "It is always cold in the South." The conditioning was to truth!)

In short, the Pavlovianization of Soviet psychology in 1950 did not wholly subordinate it to Soviet psychophysiology. Soviet psychologists have managed to find their own special areas to which Pavlov's doctrines could be extended, and, within the framework of their task, they seem to have done very well (deplorable as the imposition of a particular school of psychology might be). Certainly, taking 1940–1950 as a base line, post-1950 Soviet psychology has become manifold, growing in many directions.



Irrationally held truths may be more harmful than reasoned errors.

—T. H. HUXLEY

The Social Ethic and Rational Choosing

William H. Whyte

The Organization Man. New York: Simon & Schuster, 1956. Pp. 429. \$5.00.

Herbert A. Simon

Administrative Behavior: A Study of Decision-Making Processes in Administrative Organization. (2nd ed.) New York: Macmillan, 1957. Pp. xlviii + 257. \$5.00.

Reviewed by EDGAR F. BORGATTA

Dr. Borgatta works for Russell Sage Foundation as a social psychologist and is Adjunct Professor of Sociology in the Graduate School of Arts and Science at New York University, a man with a special interest in the psychology and sociology of small groups. He is an editor of Small Groups and used to edit Group Psychotherapy. He reviewed Bjerstedt's The Methodology of Preferential Sociometry in CP (May 1957 2, 130).

THESE volumes are related, but only in a distant sense. They concentrate their interest on a broad area, the nexus of organization, but Simon is concerned with the description and analysis of behavior, whereas Whyte presents a commentary on trends.

Whyte, an editor of *Fortune* magazine, presents as the main thesis of *The Organization Man* that our view of history requires recognition of the Protestant Ethic and its relationship to the development of industrialization and modern society. The Protestant Ethic in its more recent dress emphasizes the Horatio Alger story, going west, individualism, and vertical mobility for anyone who will work for it. While there are some remnants of this will to succeed in our value system, Whyte argues that we now have a Social Ethic, which he defines as follows:

legitimate the pressures of society against the individual. Its major propositions are three: a belief in the group as the source of creativity; a belief in 'belongingness' as the ultimate need of the individual; and a belief in the application of science to achieve the belongingness.

The Social Ethic has its importance in its growing universality among both practical people and academic people, and the prime illustration is to be found in the organization man.

The volume, thus, centers around a discussion of the Social Ethic and the organization man. Without any question, Whyte is one of the more perceptive journalistic writers who has come on the scene to deal with this topic. He has an extremely agile and facile way of placing in question some of the sugar-coated generalizations that have been readily accepted in our teachers' colleges and schools of business administration, and are common coin in some professional areas. Yet in the course of his presentation it is difficult to place Whyte. On the one hand, he argues against the romantic notion that idealized the Protestant Ethic, but, on the other, he is critical of the more modern situation. His chapter on *Scientism* seems to build too good a case for not being overly hopeful of the boons stemming from science. Thus his presentation borders on being sensational because it goes against the general trend, not merely resisting but over-

reacting and in an extremely glib manner. One might say, much as a lawyer does, that Whyte has *built* a case that leaves scientism in a relatively unfavorable light, and indeed he is extremely persuasive.

Let us assume, for the moment, [he writes], that a precise science of man is not a will-o'-the-wisp and that we are on our way to achieving it. We are left with a knotty problem. What do we do about good and evil, right and wrong? Believers in scientism confess that the question requires hard thinking. They are glad that ethical relativism has freed us from the narrow view that our own group's given values are the only correct ones. Obviously, then, a science of man could not freeze on one scheme of ethics. If we are to be governed by it, however, it would need some sort of ethics. How are we to determine just what they should be?

The problem here is, as in so many other places, Whyte's apparent unwillingness to tolerate the ambiguous or to live in the present. Do we have to answer his questions now? Better tools do not resolve ethical and moral questions, but who would deny this? And does the availability of better tools require us to suspect motives more than before?

It is often difficult to be critical of Whyte because his points are made in such a telling manner. For example, he points to the encroachments that one may feel even today and that essentially are more often defined in terms of a requirement for action than as a restraint of action. He indicates that in the 1984 of Orwell one would at least have Big Brother as his enemy, a known enemy, while today one's enemies might turn out to be "a mild-looking group of therapists who, like the Grand Inquisitor, would be doing what they did to help you."

BELONGINGNESS, according to Whyte, is something that stems from an attempt to recreate a condition that is presumed to have existed during the Middle Ages and that is related to our manorial precursors. If we may point our finger as he points his, Whyte appears to indulge in historicisms and essentially require that what people are concerned with today be reflections of previous conditions. He also tends to

capitalize on the virtue of moderation, arguing firmly on both sides of the fence.

Someday someone is going to create a stir by proposing a radical new tool for the study of people. It will be called the face-value technique. It will be based on the premise that people often do what they do for the reasons they think they do. The use of this technique would lead to many pitfalls, for it is undeniably true that people do not always act logically or say what they mean. But I wonder if it would produce findings any more unscientific than the opposite course.

This point is so telling among persons who are familiar with what may be called the 'plausible theories in psychology' that it requires considerable attention. For those who are seeking deeper and deeper understanding into the motives of behavior, this iconoclasm provides a rude warning not to neglect the more obvious.

In his consideration of the concept of *togetherness*, Whyte basically attacks the naiveté with which persons have responded to the so-called 'group dynamics movement.' He remarks on the current belief that science has proved that the group is superior to the individual, when certainly this is not the case. Some values of group behavior have been so emphasized that the individual defenses and privileges have been ignored. Further, in this new code it is so good to be cooperative that to be assertive or to oppose the dominant value of the group comes to be considered either as bad taste, or worse—a psychic disorder!

Whyte's criticism does not, however, end here, and implicitly it must go beyond. It would seem, if Whyte is correct in his identification of this Social Ethic, that its major implication is dangerous to social and psychological research, for, if the ethic is a part of the culture of those who will do research, they may be biased toward confirming their beliefs rather than toward investigating the situation. Post hoc, one may say that this bias seems to be descriptive of much of the research that has gone on in the area of group dynamics.

Whyte's description of the organization man is both provocative and insightful. It harps on a theme, however,

that the organization man no longer prepares to reach the top, but is satisfied to set his goals somewhere short of the top. He looks for a comfortable but advanced position, rather than for the top of the heap, and he looks forward to being a 'well-rounded' man, and in a pathetic sort of way he comes close to achieving this goal. On the other hand, the executive of today (as contrasted with the organization man) still 'overworks, still drives, and still has the characteristics that he had a generation ago, although the edge has been taken off him by the Social Ethic.

THUS Whyte is saying that he is against mediocrity and in favor of genius. If the youths of today are more realistic in recognizing that not all persons can be on top of the heap, they are not satisfying the idealistic model of ascendancy to the top that appeals to Whyte. In spite of his acuteness in criticizing some of our modern foibles, Whyte seems to have the ancient tendency to look at a normal distribution and suggest it is too bad that so many persons are in the middle. The criticism is not so much that the mean should be shifted up as: "Wouldn't it be nice if 50 per cent of the distribution were beyond the second standard deviation above the mean!"

The one single thing that may appeal most to the psychologist is Whyte's critique on tests. His analysis is a blistering one, and also humorous in its satirical vein. His rules for doing well on any test of conformity (personality) are well taken, and they are worth repeating in any number of places, as well as here:

(1) When asked for word associations or comments about the world, give the most conventional, run-of-the-mill, pedestrian answer possible. (2) When in doubt about the most beneficial answer to any question, repeat to yourself:

I loved my father and my mother, but my father a little bit more.

I like things pretty much the way they are.

I never worry much about anything.

I don't care for books or music much.

I love my wife and children.

I don't let them get in the way of company work.

We may summarize the review of this book by saying that it has a great deal of insightful description, which is marred by the author's journalism and his humanistic desire to improve society. While he borders on cynicism, what really troubles him is that he is a romanticist of the first water. He wants individualism, genius, and that all should strive for the top. He is cognizant of the restrictions that are placed on society by the fact of sheer physical numbers and the general advancement of education of our own cultural groups, but he seems to ignore these in the implications of how far the individual may go or aspire. He seems to resent it when a man knows that if he 'goes west' he will find Los Angeles instead of a frontier.

SIMON's book is one of those rare ones that is extremely difficult to review *because* it has the peculiar characteristic of being a superb job of description, and is of uniform high quality. In reading it, one can easily be deceived into thinking that it is superficial, because it reads so well and because it deals with generalizations at a level at which we like to think we operate and which we sometimes call 'common sense.' This combination makes an uncommon product, stemming, as it does, from a mature mind that has had access to a number of scientific disciplines so that it can abstract the essence and best theory from each. This comment does not mean that *Administrative Behavior* is profound. Rather it means that the book undertakes description at the appropriate level and in terms of the status of knowledge, that its author considers all the relevant variables within the given frame of reference (at least in so far as this reader can discern), and that he does so with attention to the limitations involved in his defining operations.

The core of Simon's book deals with human choice and rationality. He is interested in the study of organization, however, and:

In the study of organization, the operative employee must be at the focus of attention, for the success of the structure will be judged by his performance within it.

Insight into the structure and function of an organization can best be gained by analyzing the manner in which the decisions and behavior of such employees are influenced within and by the organization.

Simon compresses his description through a choice of words and directness of statement. *Organization* refers to the complex pattern of communication and relations in the human group that provides to each member much of the information that enters into his decisions—including his expectations of what others will do. Where alternatives for choice exist, all decision is a matter of compromise. Within the framework of administrative theory, or in dealing with the problem of how an organization may be constructed in order to accomplish its work efficiently, the question of decision-making may be examined. *Efficiency* is defined in terms of what is meant by good or correct administrative behavior, and the examination of the single member of the administrative organization is centered around his limits (a) in terms of his ability to perform, and (b) in terms of his ability to make correct decisions.

ACCORDING to Simon, *rationality* needs to be defined in terms of a specific frame of reference, and for administrative theory the principal frame is the organization and its proper functioning within the broader context. An important distinction in defining rationality concerns the ethical and the factual. Having clarified this distinction admirably, Simon indicates some of the criticisms of the common means-end schema that accrue from not making this distinction and also from ignoring both the comparative elements in decision-making when alternatives exist and the time-factor involved in purposive behavior. This discussion leads him to his acute discrimination between decision, or choice, which is the process of selection of an alternative at a moment, and the series of such decisions over time which he identifies as strategy. Decision, thus, involves alternative strategies and the determination and comparative evaluation of the sets of consequences. The distinction between strategy and decision is an important

one, and yet a subtle one, as may be seen in the consideration of the general types of decisions that occur:

It should be made clear that actual events are determined by choice among on-the-spot alternatives for immediate behavior. In a strict sense, a decision can influence the future in only two ways: (1) present behavior, determined by this decision, may limit future possibilities, and (2) future decisions may be guided to a greater or lesser degree by the present decision.

Within this context Simon discusses planning as involving the psychological processes of selecting general criteria of choice. He particularizes them by applying them to specific situations. Planning as a process delimits the sphere of choices, and as such is an early decision that may or may not include the "best decision."

In the course of his discussion Simon must draw heavily from the fields of sociology, psychology, economics, and government. He does so, however, with a skill that makes a sociologist look up and say, "If he knows the other fields as well as he knows mine, then I have no hesitancy in accepting his discussion." He has the virtue of being clear and consistent and, within the limits of his presentation, logical and organized. The importance of his work for psychology is multiple. On the one hand, he will make persons who are familiar with decision-making models and experiments feel somewhat uncomfortable because of the restrictive designs and naiveté of theory that usually characterize them. In fact, Simon appears already to have made his impact on this area as may be judged by his contributions, some of which are reprinted in *Models of Man* (New York: John Wiley and Sons, Inc., 1956). On the other hand, he has done a superior job of indicating the level of psychology that can be integrated into more general frames of reference than that of 'understanding' the individual. Furthermore, Simon at several junctures clearly points to the relationship of his study to the field of psychology. By way of example, he indicates that where authority is accepted by subordinates, there is no requirement for psychological propositions for understanding behavior, except in the sense of determin-

ing the area within which authority will be respected. Psychological propositions are important, however, in determining the degree to which the intent of the order-giver will actually be carried out. Thus, psychology contributes to administration as a condition, as a set of limits, rather than as a part of the administrative theory itself. Still, psychology remains important, for it pro-

vides knowledge about many of the mechanisms that are usually taken as the assumptions of what the human person is.

The Simon volume offers a pleasurable experience in lucid scholarship. It is well worth anyone's time. Whyte's volume, on the other hand, is more likely to be something that you will enjoy recommending to a naive friend.

Education: How Good Is It?

Alfred Schwartz and Stuart C. Tiedeman, with the assistance of Donald G. Wallace

Evaluating Student Progress in the Secondary School. New York: Longmans, Green, 1957. Pp. xi + 434. \$4.75.

Reviewed by ROGER T. LENNON

Dr. Lennon is Director of the Division of Test Research and Service of the World Book Company. He is an educator although not a teacher. He lives and thinks education and its problems. He knows a great deal about tests and measurement and is involved in the development and publication of educational and psychological tests. He helped the Air Force with personnel testing in the late War, and he has been in his present post for ten years.

HERE is a book that takes its place in a rapidly growing list of titles in the realm of evaluation and measurement. It is offered as an introductory text for use at the undergraduate or graduate level and is directed primarily to the classroom teacher, present or prospective. The authors have sought to portray the role of evaluation in the educative process, spelling out its relations to instruction, administration, and guidance, and to familiarize the student with the instruments and techniques most commonly used to obtain the data needed for sound evaluation, like tests, questionnaires, and observation. They have labored to keep the treatment 'nontechnical,' shielding the reader as far as possible from any chilling exposure to statistics or psychometric theory. Dr. Schwartz is Professor of Edu-

cation at the University of Delaware and Dr. Tiedeman has the same title at Drake University.

Evaluation in education, we are told early in the present work, is "the process of judging the effectiveness or worth of an educational experience as measured against instructional objectives." This is a sensible, if not universally accepted, definition, and the reviewer wishes only that the authors had clung tenaciously to it throughout. Unfortunately their presentation of the history and function of "evaluation" bears witness to the confusion into which this term has fallen. The very title of the book implies that what is "evaluated" is student progress, not an educational experience. Before the first chapter is over, we find *evaluation* being used synonymously with *appraisal* and even with *measurement*, despite disclaimers of this identity. In Chapter 2, we are told that evaluation is to "determine the present status of the individual . . . and to determine his potentialities for further growth." But, for all this fuzziness, it must be said that the opening chapters succeed moderately well in establishing the role of evaluation in an educational program, describing, as they do, common evaluative techniques and outlining the scope, frequency, and conduct of evaluative activities. In-

cluded, too, is the familiar brief history of "evaluation," not without some minor inaccuracies. (Tyler, Wrightstone, *et al.* will be surprised to learn that "World War II created the conditions from which the evaluation movement evolved.")

The book recognizes that the place to begin a discussion of evaluation is at the ends of education—that is to say, with goals or objectives. Accordingly it sets about considering how educational objectives are established, and how general objectives may be translated into classroom goals defined so specifically that progress toward them can be measured or evaluated. The reviewer has his doubts that the average student, on the basis of the two-chapter discussion provided, will be able to produce a respectable list of classroom objectives (and the fact, of course, is that a great majority of teachers take their objectives ready-made from some other source); but the discussion should succeed in developing an appreciation of the importance of specifically defined goals as prerequisite to worthwhile evaluation.

Does the book provide the background necessary for evaluating and using properly the evaluative devices themselves? Chapter 5 offers a systematic treatment of the concepts of *validity*, *reliability*, *objectivity*, *efficiency*, and *usefulness* as applied to evaluative techniques. The discussion of these concepts is, on the whole, undistinguished and marked at times by a looseness that is not to be excused on grounds of simplicity or readability. Witness such statements as:

Many of the existing paper-and-pencil intelligence tests have been validated by computing coefficients of correlation between scores on the new test and scores obtained from the Stanford-Binet Scale. [That notion was only a half-truth years ago.]

Every student received the same score on the second administration as on the first. This test, we could then say, is 100 per cent reliable.

The reviewer confesses to a general prejudice against efforts to relegate statistics and measurement to a minor role in treatments of evaluation, and the volume under review has served to re-

inforce this bias. The handling of statistics, always a troublesome matter in the introductory text on measurement or evaluation, is here undertaken in a relatively brief chapter that comes late in the book. The discussion of topics which the reviewer deems essential to proper understanding and conduct of evaluation activities is sketchy. For example, scarcely a page of text is devoted to the matter of norms.

The central role of teacher-made tests as sources of evaluative data is reflected in a three-chapter treatment of the construction of such tests. Detailed and specific suggestions are given for the preparation of a variety of both objective and essay-type questions. An abundance of sample items illustrates both good and poor practice in item writing. Helpful recommendations are given concerning development of pools of test items, maintaining records of item performance, reproducing tests, and many practical aspects of test construction that confront the teacher. This section should prove one of the most useful in the book.

To this reviewer, the facts of current school practice would have suggested a somewhat different apportionment of the space allotted to standardized tests and to other evaluative devices such as sociograms, check lists, questionnaires, observation, and anecdotal records. For good or ill, these latter devices are used with far less frequency by the secondary school teacher than are standardized tests. Surely it would be advantageous in a book on evaluation to describe in some detail at least a few of the more widely used achievement batteries and subject tests. Lists of some such tests are given, but there is little discussion or critical analysis of them. By contrast, the treatment of sociometry, the case study, and the interview is extensive beyond the needs—and probably beyond the interest—of most teachers.

In one important respect the book fails to live up to the promise of its title. There is little in the book that makes it uniquely appropriate for the secondary school teacher. Not a few of the illustrations are from elementary tests, or pertain to practices that are

ordinarily identified with lower levels. To be sure, there must be a common core of material in any volume on measurement or evaluation, for whatever level it is intended; but, if a course is to be offered, or a text written, that purports to be particularly appropriate for the secondary teacher, there are certain issues that should be dealt with at some length. Included would be such matters as the relative importance of measurement of generalized outcomes vs. specific course outcomes; content validity and appropriateness of norm populations in relation to the varied curricula pursued at the secondary level; implications of the greater emphasis on the use of tests for educational and vocational guidance purposes at the secondary level; the relative merits of multifactor and general ability tests at the secondary level. These items receive but scant attention here.

For a book which makes special claim to "readability" and clarity, *Evaluating Student Progress* seemed to this reviewer to fall short of excellence. Needless repetition and duplication is to be found. The book teems with lists of advantages, disadvantages, observations, rules, and dangers. The authors' passion for enumeration reaches its height in Chapter 17, where, on every page but one of a 15-page chapter, there appears a listing of one kind of another. Not only does such cataloging make for dull reading; it is also symptomatic of a general weakness of the book—a tendency to 'tell,' to seek to convey information rather than to develop understanding or skills. There is no provision for student involvement or activity—no exercises, no problems, no questions, only the briefest of bibliographies. Greatly will this book need an instructor to supplement it.

When Wishes Are Not Horses

Donald Davidson and Patrick Suppes, in collaboration with Sidney Siegel
Decision Making: An Experimental Approach. Stanford, Calif.: Stanford University Press, 1957. Pp. 121. \$3.25.

Reviewed by EUGENE GALANTER

Dr. Galanter is Assistant Professor of Psychology at the University of Pennsylvania. He likes his psychology with the mathematics left in and he is working right now on sensory scaling and decision-making. He reviewed Bush, Abelson, and Hyman's Mathematics for Psychologists (SSRC, 1956) and Kemeny, Snell, and Thompson's Introduction to Finite Mathematics (Prentice-Hall, 1957) in CP, Dec. 1957, 2, 315f.

ONE omnipresent feature of the behavior of organisms is their selectivity of action. Much of one's mental life consists in choosing among alternative courses of action, and it is doubtless true that some scheme underlies the choices. The task of finding and testing a theory that will make people's choices intelligible is a problem of first rank. The authors of this slim but meaty

volume have set their sights on precisely this end, and, with a powerful arsenal of modern mathematics, give the problem a fair chase.

Davidson and Suppes, both Stanford University philosophers, have attended to the formal aspects of the problem; they were assisted by Siegel, a psychologist at Pennsylvania State University, in putting the theory to test. The authors have presented the most thorough part of their work in the first two chapters, which comprise 81 of the 117 pages of text. The two final chapters deal respectively with a second theory and experiment, and with a theoretical question not analyzed in the first portion.

Three related problems are presented in the first two chapters. First, choice behavior is characterized, and the authors propose a formalization or model which purports to represent abstractly

their description of choice behavior. They then provide an empirical interpretation of their model designed to relate experimental facts to the abstract elements of their model. Finally, they provide a test for the conjunction of the model and the interpretation.

THE choices they study are those between alternative courses of action whose outcomes are more or less preferred and whose likelihood of attainment are less than certain. It is assumed that two aspects of the relation between the person and the alternatives determine the choice. One is the desirability of the outcome. The other is the degree to which the person believes the desired outcome can be attained. It is proposed that numerical values can be assigned to each of these components in such a way that a simple mathematical composition predicts the choices. Specifically, given a scale of preference, a utility scale for outcomes, and a measure of degree of belief, or scale of 'subjective probability,' the person will select of two alternatives the one having the larger subjective expected utility. Note that this formulation implies that the person must discriminate perfectly between the alternatives.

Assuming this theory correct, it is not obvious how, from data on choices between alternatives, to effect a decomposition into the two subjective scales. To solve this problem the authors resort to the artful technique of divide and conquer. First, they must find an event with the property that its occurrence and non-occurrence are equal in subjective probability. This selection allows them to cancel the term for subjective probability from their equation, leaving only the utilities. They can then measure the utilities of the outcomes. (The generality of the theory is severely limited, however, since the outcomes that they have to find must be equally spaced on the utility scale.) Second, by using these equally spaced outcomes (amounts of money) in new combinations, they can check their hypothesis and predict new choices. With these considerations out of the way they turn to the experimental problems.

Problem One: Can they find an event and its complement with equal subjective probability? They report that they can and have. The authors constructed a special die having one low-association nonsense syllable on three faces, and another such syllable on the other three faces. This die was tested in their experiment by varying the outcomes a penny at a time, showing that a change of one cent would reverse the choices that their subjects made. (Unhappily, the data from this ingenious experiment are not presented, although on other occasions the authors present abundant data.) Variations of one penny could produce a change in expected utility at least as great as 10% if one can infer that this experiment used sums of the same magnitude as the major experiment they report later in the book.

Problem Two: Can the scale of utility be constructed and used to predict future choices? Davidson, Suppes, and Siegel report that for fifteen of their nineteen subjects a scale was constructed that satisfied the theoretical restrictions, and that it could predict choice behavior for seven out of eight subjects that were tested later. In sum, their results show that, for very small sums of money, utility is not linearly related to money. Rather, there is a falling off of marginal utility.

UP to this point the authors are dealing with a theory that requires perfect discrimination between alternatives. That such precision is not always found hardly needs verification. To deal with imperfect discrimination we may take one of two tacks. We may assume that the underlying discriminations are perfect but that an error term must be appended to it, a procedure that leaves the theory algebraic and *ad hoc*. The other course is to assume from the start that discrimination is fundamentally stochastic and to construct a probabilistic theory. This direction seems to me to be the more fruitful, since with a theory of this kind the variability as well as the mean values is a proper datum to describe.

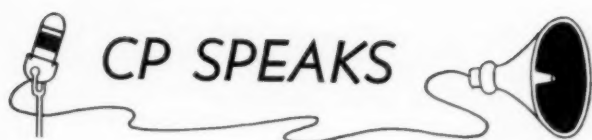
The authors are sensitive to this problem, and, in Chapter 3, suggest a theory that attempts to cope with the ubi-

quitous and perhaps intrinsic variability of behavior. In essence, the theory adopts the tactic of adding to the utility-scale values an error term which makes a 'best fit' possible from any data. The hitch here is that the ruse leads to measures of 'error' whose order of magnitude is as large as the intervals on the utility scale.

The final chapter treats of incomparable outcomes and consists of notes toward a theory of multidimensional utility. This chapter also considers the associated problem of characterizing the utility of gambling when one of the alternatives is a sure thing. The brevity of this section and its lack of rigorous interpretation mark it, at most, as a working paper or a progress report.

MOST of the technical criticisms that can be directed toward this book have already been made by the authors. Let me comment on only those aspects that the authors could not be expected to discuss. The goal of formalizing the behavior of people in a quantitative and rational scheme is so profoundly important for psychology that every endeavor in this direction deserves highest praise. Yet, for a tentative step of this kind, one would prefer the opportunity to work on these problems as they appear in the periodical literature, before seeing, between hard covers, a full-blown presentation with admittedly sketchy support. A weakness of such prematurity—in this instance—is the relative triviality of the experiments in comparison to the mathematics. The data, here presented, are very small game for the artillery. Finally, there is the question of whether this approach, attributable most directly to Ramsey, and to von Neumann and Morgenstern, is the best route to the solution of the problem. Recent developments in psychophysical scaling suggest alternative possibilities.

The authors have undertaken to explore a formidable and important problem. They bring before psychologists new and intriguing ideas from other fields. They resort to experimental evidence, and tell a story that is worth hearing. Yet one can wish that they had waited a bit before publishing in this form.



WHEN POOR WRITING IS GOOD

SCORE for John Ciardi, *CP* remarked (Nov. 1957, 2, 279), with tongue then very much in cheek: "Active participation is one of the most important conditions of learning. Should not the writers of textbooks try to be obscure—though intelligible—so that the student, having finally figured out the meaning, will never forget it?" Ciardi had been defending the obscurity of modern poetry on the ground that it gives the reader the fun of discovering what the poet meant—like any other puzzle.

Now here comes Austin B. Wood with the suggestion that this suggestion may actually have merit in it for textbooks as well as poetry. He used, with irregular alternation through more than one edition, two textbooks in elementary psychology, *P* and *Q*. *P* was beautifully written, so clear that the students never had to read a paragraph twice. It even improved Wood's understanding of some of the topics treated, or so he thinks. *Q* kept puzzling the students so that they had to read and reread sentences, paragraphs, and chapters. Wood was thoroughly sold on *P*. But just look! The students did better with *Q*, better in examinations and in the writing of comprehensible essays based on *Q*. He does not like to believe his own conclusion but he is a scientist and he sends *CP* this molecule of truth.

Another molecule is S. W. Morris' *Signs, Language and Behavior* which S. S. Stevens and G. C. Stone gave the next to the highest Flesch count in a list of 17 books (*Amer. Psychol.*, July 1947, 2, 230-235). George Miller used this with a class and it worked fine because they worked hard.

Come to think of it, this kind of function is not new. Do distractors diminish accuracy? Sometimes they improve accuracy, because, since they are

known to tend to interfere with attention, they may spur the attention to make the distracted guy do better (A. J. Hamlin, 1896; E. E. Cassell and K. M. Dallenbach, 1918). Cities are wonderful places to work: they're so noisy. People spur themselves on to great achievement by protesting how the noise disturbs them. If the distractor really distracts, then you are out of luck. You do not write the best poetry while you are having a child without analgesia. But if the distractor is a spur, like a hard chair at the desk, then you may get a profit out of the distractor that spurs instead of distracting.

This matter needs more attention in these days when America is going soft by prosperity and progressive education. Maybe *CP*'s on the wrong track. Perhaps it ought to get dour and muddy the reviews a little instead of filtering them. This predicament is, of course, an all-or-nothing one. If you get over the threshold, all right; if not, too bad. And the higher the threshold, the more glorious the result if you get over it. It's like humor. The harder it is to see the point, the more fun it is if you see it, but how dull if you don't! A low threshold for wit gives pleasure to a great many; a higher threshold much more pleasure to but a few. Presumably the humorist estimates his audience, and then adjusts the threshold to maximize the total fun. So the textbook writer, for a given audience, could work for maximal learning by carefully varying the amount of mud he introduces into the stream of his otherwise crystal-clear exposition.

THE INJURED AUTHORS

NOT graven in stone but merely in its mutable neural engrams *CP* has been hanging onto its list of Injured Authors, the authors who think—indeed are convinced—that they have been unfairly

reviewed in *CP*. Last August *CP* explained about its immediate policy in respect of justice (*CP*, Aug. 1957, 2, 210f.); it wants justice, does not expect to obtain it, offers a servomechanism whereby the injustice can be diminished though never completely abolished. *CP* has, however, been impressed by the smallness of its list of Injured Authors, who are, of course, only those who emerge above *CP*'s threshold. Maybe there are dozens of others stewing in the bitter juice of being misunderstood, others that *CP* never hears about. But would you not like to know how many persons are on this list for 1956 and 1957 taken together? Here it is in tabular form.

Injured Authors who wrote to ON THE OTHER HAND saying why. *CP* encourages authors to rebut reviewers. If they have a case that can be stated in words, they have also a responsibility to the profession to make it aloud, not to hide it behind their own feelings. If they have no case, they find that out by trying to formulate the case in words 5

Authors presumably Injured, until a knight in shining armor rode into the lists of ON THE OTHER HAND. *CP* suggested this procedure in January 1956 (*CP*, 1, 29) as avoiding ego involvement by the rebutter, the champion. If there is connivance between author and rebutter in advance, *CP* does not know it. *CP* never taps telephones. This figure includes two pairs of joint authors, each pair with a single injury, and one author who had already defended himself once before his belated champion arrived 8

Authors Injured enough to write a letter or two, but not enough to tell the world, and still without a volunteering champion. These are those who were urged by *CP* to make their cases in ON THE OTHER HAND and who successfully resisted the pressure because of pacifism, shyness, or some comparable personality trait 4

There remain, of course, the silent and hurt, who did not write to *CP* and to whose defense no one jumped. Two publishers, not included in these figures, protested, but they are in business and

not expected to transcend egoism in the interests of truth. There was one case where the author's protest was about an unwritten review; the author had found out who the reviewer was to be and in vehement successive letters gave *CP* its prize protest, all before the review was even written.

There were three requests that *CP* print another review of the same book, on the assumption that you can not draw two black balls out of the same bag. Indeed there are those who would like all the reviews in *CP* doubled—two reviews by two reviewers, always a spare in case of a puncture. To this suggestion *CP* sits back and shakes its head. What would happen as the double unfavorable reviews began coming out? Some books are not good; they would merit double condemnation if assessment were in general to be doubled; but how *CP* would hate to find itself feeding an Injured Author a second rebuke. Mostly the I. A. assumes that a second rebuke is an impossibility; he worked so hard and long, how can honest effort be justly depreciated?

Yet just look at the big statistic. Here are 16 authors of 13 books unfavorably reviewed, as the authors understood the reviewer's comments, and that is out of a total of 425 reviews printed by *CP* in 1956 and 1957. Only 3.1%. Maybe *CP* does not hear all the complaints about reviewers, but just now, with this datum before it, *CP* thinks its reviewers have in these two years done a pretty good job. To them it says: *Vos omnes benedicimini!* (That, if you do not know, is a plural imperative benediction to all of them.)

EDUCATIONAL PSYCHOLOGY IN THE U.S.S.R.

IN order to let the English-speaking psychologists know what the Russian-speaking ones are up to, *CP* keeps breaking its rules about all reviews being critical and not abstracts, about reviewing only quite recent books. It did that with Razran's review of Russian books for 1950-1956 (*CP*, Apr., 1957, 2, 93-101), which one correspondent complained about as being outside *CP*'s function. Now Dr. Richard K. Overton of Fort Hays, Kansas State College,

writes in about a book Razran mentioned but did not consider in detail:

Zaporozhets, A. V. *Psikhologiya*. (2nd ed.) Moscow: UchPedGiz, 1955. Pp. 188. 3.90 rubles (98 cents).

Overton has translated this book into English for his own edification, and *CP* thinks what he has to say about it is worth quoting.

This little volume is probably the most widely used educational psychology text in the Soviet Union. A six-word note on the title sheet announces the book's purpose, and its third printed page introduces future teachers to physiology and "the dialectic materialism of Marx and Engels, as developed by Lenin and Stalin."

Having cast his verbal pearls before the Commissars, Zaporozhets rapidly describes the nervous system and Pavlov's conditioning paradigm. Surprisingly, Zaporozhets avoids a highly mechanistic interpretation of conditioning. Instead, he describes the conditioned stimulus as a "signal" that the unconditioned stimulus is coming. It is by way of "signalling systems" that he gets to his discussion of non-materialistic topics: perceiving, remembering, thinking volition, and personality.

All the chapters are blunt, brief, and relatively straightforward. If it were not for its superfluous references to Communist heroes, an English translation of the book might appeal to those American teachers who esteem pithiness and physiology.

THE CEEB SUPPORTS RESEARCH

THE College Entrance Examination Board wishes the readers of *CP* to know that there is now available a volume called *The Research Activities of the College Entrance Examination Board, 1952-1957*, a volume which contains a nontechnical discussion of the more than two hundred studies supported by the Board during these half dozen years. The reported researches pertain to test construction and validation, college admissions practices, the nature of the pupil-candidate-student universes, the nature of the school-college universes, and CEEB programs. The volume is designed to inform researchers of the findings of Board-supported studies and to elicit their suggestions for further research in which they may be interested. Persons desiring a copy of this report should write to Dr. Joshua A. Fishman,

Director of Research, College Entrance Examination Board, 425 West 117th Street, New York 27, New York. Adults with serious intentions will not be billed.

ON-GOING STRUCTURES

THE effective, ultimately successful scholars have vectors. And that for them is the same thing as knowing what is important. For Floyd Allport the theme is structure—generalized "on-going structures," "event structures." He had a chapter on the general theory in his *Theories of Perception and the Concept of Structure* (John Wiley, 1955), a book that someone begged *CP* to review, when *CP* could not because the *Psychological Bulletin* had already done it. Now Allport is going beyond perception, hoping to manage a book on the general theory of structure itself, not psychology merely, all science, although he will apply it to psychology. He probably will call it *Structure in Nature*. It is hard work and he does not know how fast he will get on, but *CP* thought you would like to know about this undertaking because his other book was so good. He has just retired and is trying to find out whether ideas grow better in California than in the east.

MORENO—GROUP PSYCHOTHERAPY

HERE is a note about group psychotherapy from Robert W. White.

"Dr. J. L. Moreno has issued a book under the title *The First Book on Group Psychotherapy, 1932*. It is a reissue, with a long argumentative preface and a short postscript, of a monograph he prepared in 1931, *Application of the Group Method to Classification*, for presentation to a national conference of prison workers. It was discussed the next year at a round table at the American Psychiatric Association; the whole discussion is included in the present volume. Dr. Moreno claims to be the founder of group psychotherapy, and this book, available from Beacon House (P.O. Box 311, Beacon, N. Y., 138 pages, \$3.50), will be of interest to scholars who want to weigh the historical correctness of his claim."

—E. G. B.



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Bless Thee, Linton, Thou Art Translated!

Ralph Linton

Culture and Mental Disorders. (Ed. by George Devereux.) Springfield, Ill.: Charles C Thomas, 1956. Pp. vii + 139. \$4.50.

Reviewed by MARVIN K. OPLER

Dr. Opler has been teaching and conducting research on the topic of this book for the past six years in the Department of Psychiatry of the Cornell University Medical College in New York. He is the author of *Culture, Psychiatry and Human Values* (C. C. Thomas, 1956) and one of Ralph Linton's co-authors in *Acculturation in Seven American Indian Tribes* (Appleton-Century-Crofts, 1940). He describes himself as an anthropologist who has worked in social psychiatry since 1938, who was a student and friend of Linton's, and who began publishing in psychological journals on these same topics in 1942.

ANY resemblance between Linton's 1953 Salmon Lectures, given in New York, and this wildly speculative book published three years later is wholly accidental. The book is edited by George Devereux from Linton's incomplete notes and lecture outlines remaining after his death. Devereux is an anthropologist who is concerned with psychoanalysis. (See the review of his *Therapeutic Education: Its Theoretical Bases and Practice*, CP, Jan. 1957, 2, cf.) Cora DuBois, who collaborated with Abram Kardiner and Linton, already has summed up leading opinion, and this reviewer's own poll could do no better. After more than two pages of a devastating review in the *American Anthropologist* (vol. 59, Feb. 1957), DuBois wrote: "Had Linton lived, it seems unlikely that he would have sanctioned this publication in its present form." The reader is referred to her review for several substantive criticisms leading to this conclusion.

This embroidered version of the lectures is primarily about Devereux. When I looked over Linton's notes and outlines, with Linton in New York in 1953, and when I took copious notes on the lectures at the New York Academy of Medicine, the original materials were quite different from what Devereux has given us. Of course, Linton was no epidemiologist of mental disorders, and his period of exposure to medical psychiatry was limited. He did not work in medical school or hospital settings. Much of what he did learn came from association with Professor Kardiner of Columbia University. While Kardiner's volumes, with which Linton is associated (*The Individual and His Society* and *The Psychological Frontiers of Society*), receive criticism in terms of Devereux's review (p. 42), DuBois receives only two references and Kardiner a few more. Sapir, who preceded Linton as Sterling Professor at Yale, likewise gets two, neither of which do Sapir justice. Freud rates three, whereas Devereux has thirteen indexed references. In the bibliography, only three of Kardiner's works are listed, and six of Linton's (including one labeled "Manuscript—no date"), but Devereux, who was not mentioned in the lectures at the New York Academy of Medicine, is cited seven times in this bibliography.

Presuming we are all so adult that we do not need to count references and bibliographic items, we may ask what are the contents of these references to Devereux? Pages 75–80 put into Linton's mouth a continuous, uninterrupted eulogy of Devereux, beginning, "In my first lecture I cited extensively Dev-

ereux's views on the crucial importance of the oedipal period in the formation of that segment of the personality which may be called the 'basic personality.' In the same work, he also made two other points which I propose to utilize." The eulogy by the editor of himself ends five pages later with: "I have leaned toward Devereux's views which form a consistent scheme." I can find no one, myself included, who believes that Devereux was referred to at all in these contexts of the lectures and the original outlines. DuBois quotes in full from a passage on page 78 with a wry comment. The Kardiner books, which used the concepts of 'basic personality,' are dated 1939 and 1945, respectively, whereas in the passage just quoted and in alluding to Devereux's "same work," there is a footnote to Devereux's *Reality and Dream* which, astoundingly—for anyone who wishes to trace the development of the concept of "basic personality"—did not appear until 1951. Still the distortion is consistent. Devereux has already starred in the first pages of what purport to be the opening lecture (pp. 10f.), and then quoted for almost two solid pages (pp. 12f.) from his own writings.

The result is, of course, neither an account of the Salmon lectures nor a fitting memorial to Linton. If the reader wishes to learn anything of Linton's point of view, he ought to read *The Cultural Background of Personality*, which Linton saw through to print in 1945, and which has really but little on mental illnesses. To supplement it, he will have to turn to the massive volumes by Kardiner and to DuBois' *The People of Alor*.

This review is undertaken with considerable reluctance and after urging by former associates of Ralph Linton. The reviewer earlier declined to discuss the book in another journal. Despite their friendship for Linton, his associates confess to considerable misgivings about his original lectures, which were undertaken in a period of illness that proved fatal. Distortion has, however, no place in the history of science, and a friend could do no more when posthumous accounts are involved. He should also do no less.

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The Macmillan Company

60 FIFTH AVENUE, NEW YORK 11, N.Y.

The Clinical Psychologist Takes Stock

Sol L. Garfield

Introductory Clinical Psychology: An Overview of the Functions, Methods, and Problems of Contemporary Clinical Psychology. New York: Macmillan, 1957. Pp. xiii + 469. \$6.00.

Reviewed by RICHARD L. CUTLER

Dr. Cutler, who is cataloged as Assistant Professor of Psychology at the University of Michigan, seems to be a quadruped, with one foot in the theory and practice of clinical psychology, another in general psychology in the region where perception, learning, and cognition are found developing in children, a third in teaching which means explaining any hard subject to any bright listener and includes orienting graduate students in clinical psychology, and the fourth in administration in the everyman's land between clinical and general psychology. In other words, he is interested in personality theory and thus will some day write a book about personal adjustment.

THE character of clinical psychology, as a field which is at once artistic, intuitive, scientific, and objective, makes it mandatory that the author of an introductory survey of the field be a person of catholic inclination. In this respect Dr. Garfield qualifies admirably. His experience in the Veterans Administration, both as a practicing clinician and as an administrator, has brought him into close contact with both the practical problems of the discipline and also the issues underlying the training of professional personnel. His university experience at Connecticut and Northwestern, plus his demonstrated research interest and competence, have given him respect for his academic colleagues and earned him their respect. His book reflects his breadth and sophistication, and usually it rises above the danger of pedestrian-

ism inherent in the writing of an introductory text.

The strengths, weaknesses, and concerns of clinical psychology find adequate reflection in this volume. An attitude of rather self-conscious professionalism, which has characterized the field in the post-war years, is manifest throughout. This attitude seems to have two major roots. The first lies in the clinician's continuing doubt about his own professional adequacy, particularly with reference to his relationships with psychiatry and to the issue of private psychotherapeutic practice. The second is his concern about scientific responsibility and rigor vis-à-vis his academic brethren.

Encouragingly, however, one finds in Dr. Garfield's approach to these problems neither the helpless trepidation of the young child, nor the militant assertiveness of the adolescent, which have characterized clinical psychology at earlier stages of its development. Rather, there is the clear suggestion that the seeds of maturity which were sown at Boulder and nurtured at Stanford are now growing into a sturdy young adult. The statement of conditions surrounding the private practice of psychotherapy by psychologists is lucid, objective, and founded in the realities of social demand and medical reluctance. To the advanced undergraduate, who is contemplating a career in clinical psychology, this book offers a rich fund of information, which is based not upon conditions as they might be or should be, but as they are.

With reference to the relationship



SOL L. GARFIELD

of clinical psychology to the science of psychology, the book is equally unpretentious and objective. One finds in it the firmly stated belief that research is a necessary function, but there is no suggestion that the service function of clinical psychologists in agencies should be subordinated to the need for research. Occasionally the impression is conveyed that research, particularly in the area of the evaluation of our diagnostic instruments, has provided us with a more substantial foundation for practice than may actually be the case. This faint exaggeration is due, not to any failure on the author's part to recognize the limitations of validating studies, but to an occasional unfortunate juxtaposition of his statements, one of which relegates to a less salient position in the presentation the final appeal to clinical judgment and intuition.

The research function of the clinical psychologist is clearly recognized by Dr. Garfield. He fails, however, to point out that university research and agency research are necessarily supplementary to each other, and that the need for co-operative research is now more pressing than ever before. The service-oriented psychologist has available a fruitful source of stimulation and hypothesis in his face-to-face dealings with patients. Inasmuch as the press of his service activity is often such as to prohibit his

engaging in extensive formal research, a statement of the potentially productive relationships which can be brought about between university researchers and front-line clinicians would have been worth while.

The reviews of research studies pertaining to the diagnostic and therapeutic tools, the stock-in-trade of the practicing clinical psychologist, vary in quality directly with the state of affairs in each area. Dr. Garfield points out that the articles reviewed are not meant to be exhaustive of the particular field, but merely exemplary. For instance, the review of research on intelligence tests is excellent; it provides a sophisticated lay knowledge of the appropriateness of their application to various clinical problems. On the other hand, the account of the methods for personality appraisal, both objective and projective, is inconclusive and occasionally unconvincing. The review of research in psychotherapy is somewhat incomplete, and, unlike the other two, does not sufficiently point up the salient issues in the field. In general, however, the author's analysis of the state of research in these three areas leads to a cogent statement of those pressing research problems, the solution of which would provide the more immediate benefit to the practitioner.

THE organization and presentation of the book are excellent. Dr. Garfield's writing style is lucid, and the reader knows at any moment what the relationship of his present concern is to the over-all plan of the book. The best sections are those concerned with the history and problems of the field, plus the description of settings in which the clinical psychologist functions and the roles that he plays in these settings. Of better-than-average level, in terms of thoroughness of coverage and reflection of the state of the field, are the sections on diagnostic tests and the research studies relating to them, although an occasional advocate of a specialized diagnostic instrument, such as the Bender-Gestalt or Blacky, will be disappointed that no mention of these techniques is made. The chapter on tests in educational and vocational guidance is dis-

appointing, providing little more than a listing of the availability of such instruments, and indicating little of the application of these specialized tools to the more sophisticated appraisal of personality.

Clinical psychologists who function outside the classic settings of neuropsychiatric hospitals, mental hygiene clinics, and general hospitals will be dissatisfied with the lack of emphasis upon their particular roles. Very little attention is paid to the functioning of clinical psychologists in education, industry, and other highly specialized settings. Lacking also is a statement of the possible extension of the clinical psychologist's role to such fields as community mental health, hospital and industrial administration, and human systems research.

The academic clinical psychologist will be somewhat dismayed that more attention is not given to the importance of the construction and testing of personality theory, an undertaking to which the practicing clinician can contribute much. Neither is there sufficient emphasis upon the importance of theory to sound practice, nor upon the implications of better understanding the intuitive processes upon which practicing clinicians base such a substantial part of their activities and recommendations.

The author is clearly aware of these omissions and of the relative emphases he places upon actual conditions of clinical practice. His justification is, of course, that an introductory text cannot legitimately be expected to cover all of the more sophisticated issues in the field, and that his selection was intended to provide an objective reflection of the discipline as it is currently practiced. In this goal he has succeeded more than adequately.

This book should find acceptance in the field and be useful to teachers of survey courses in clinical psychology at the undergraduate and beginning-graduate level. It is not intended as a manual which will teach skills, but rather as a text whose aim is to provide the beginning professional and the sophisticated layman with an orientation in the field. For this reason its omissions and emphases are not damning and it serves its purpose well.

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The Magic of the Dance

Elizabeth Rosen

Dance in Psychotherapy. New York: Bureau of Publications, Teachers College, Columbia University, 1957. Pp. xx + 178. \$4.50.

Reviewed by RODERICK W. PUGH

who is Assistant Chief Clinical Psychologist for Psychotherapy in the Veterans Administration Hospital at Hines, Illinois. He is also a Diplomate in Clinical Psychology and has been concerned with the theory and practice of psychotherapy for the last ten years. He says that basically his thinking is Sullivanian, but it is plain that Carl Rogers, with whom he was trained, has had great influence upon him. It is to Rogers that he attributes his conviction that "psychotherapy is a systematic process which must eventually stand upon a sound experimental verification" of its hypotheses and methods.

THIS monograph in the Teachers College Studies in Education series has two stated objectives. The first is to explore the value of the dance as a therapeutic adjunct in the treatment of psychotic patients. The second is to make available a collection of behavioral observations on patients as they participate in a dance program, with an account of the actual methods and materials used in the sessions. What contribution this research realizes is in terms of its pursuit of the second objective.

The inescapable impression is that prior to this study the author had minimal professional contact with hospitalized psychiatric patients. Her conscientiously detailed observations and frankly stated reactions to psychotic behavior, including the development of both heartening and disappointing insights, constitute a report of a learning experience in working with disturbed individuals. Persons experienced in inpatient psychiatric treatment may easily consider these accounts artless and re-

dundant. Nevertheless, beginning students and volunteer workers in the adjunctive therapies should find them of value, for the relevant passages effectively anticipate the usual difficulties in establishing initial contact and maintaining rapport, and they may also help to prepare the novitiate emotionally for exposure to behavioral phenomena which will tax his capacities for understanding and acceptance.

IN its exploration of the therapeutic potentialities of creative dance groups, the choreography of this study is disappointingly uncreative. It has not progressed beyond the original routines of Marian Chace whose contributions are duly acknowledged by the author. Guided by extensive experience and Sullivanian theory, Chace outlined the principles, methods, and hypotheses in the *Bulletin of the Menninger Clinic* (1953). Here there is no elaboration of the principles, improvement on the methods, or definitive testing of the hypotheses. Since the present study is essentially a duplication of Chace's work by one who appears relatively less experienced, less extensive publication would have seemed more appropriate.

It is the methodological looseness of this research that merits the strongest criticism. Such an exploratory effort, lacking even the most rudimentary controls and utilizing unstructured clinical observations as data, can at best serve the heuristic purpose of hypothesis finding, not of hypothesis testing which seems to have been the author's unfulfilled intention. Her doubts to the contrary, there are methods applicable to research in therapy which could have insured greater success.

Nevertheless the deficiencies of this study as a book and as research do not prevent its reinforcing the clinical impression that the dance has therapeutic value as a nonverbal approach to certain regressed patients.



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What Drugs Do to Animals and Tissues

Harold A. Abramson (Ed.)

Neuropharmacology. (Transactions of the Third Conference, 21-23 May 1956, Princeton, N. J.) New York: Josiah Macy, Jr. Foundation, 1957. Pp. 381. \$4.50.

Reviewed by SHERMAN ROSS

Dr. Ross, who is Professor of Psychology at the University of Maryland, is an outstanding psychopharmacologist in these days when the importance of the effects of drugs on behavior is becoming so clearly recognized. Recently he spent a year as Assistant Chief of the Psychopharmacology Service Center of the National Institute of Mental Health, and he is now the principal investigator in an NIMH project on comparative psychopharmacology. It was an NIMH research grant (M-1604) to the University of Maryland that made this review possible. Dr. Ross knows animals as well as drugs, for he is a Scientific Associate of the Jackson Memorial Laboratory at Bar Harbor, Maine, and used to be a Research Fellow of the New York Zoological Society.

SOME time ago Ralph W. Gerard of the University of Michigan remarked that conferences about drugs had become a drug on the market. It is certainly true that there have been many conferences involving different performers and yielding differential results. The reports of these meetings, frequently delayed, appear for several purposes. Some reports are in essence abstracts of the business of the conference. Others try to communicate the results, findings, conclusions, or agreements of the conference. There is a small group whose stated purposes involve some change in the efforts and attitudes of the participants or of a broader audience. These Macy Foundation conferences do have such a purpose. Frank Fremont-Smith, Medical Director of the Foundation, says that the purpose of the conference is "the

promotion of communication, the exchange of ideas, and the stimulation of creativity among the participants." The purpose of the publication of the report is to share the conference process with a larger audience.

Neuropharmacology represents one of the exciting frontiers of contemporary pharmacology. In this report informal presentations are made by some of the pioneers who are busy carving out pathways in the wilderness of neural functioning and organization. The participants include such well-known contributors as Harold A. Abramson, Murray A. Jarvik, Curt P. Richter, Jules H. Masserman, J. H. Quastel, Henry K. Beecher, Joel Elkes, Dominick P. Purpura, Bernard B. Brodie, and James Olds. The remainder of the discussion group are a selected group of the stars of modern biological science, including, among others, Seymour S. Kety, Horace W. Magoun, I. Arthur Mirsky, Amedeo S. Marazzi, with Hudson Hoagland as chairman. To me it is one of the important open questions as to whether or not such gifted, enthusiastic, and essentially independent workers can be further stimulated from an already high level of creative productivity.

The informal presentations differ widely in their starting points in scope, in method, and in stimulation value for the participants. The largest single piece, over 100 pages, is by Richter on the production and control of alcoholic cravings in rats, a report which evoked a great deal of discussion. The reports and discussions cover a wide array of topics, from snails to man, from salt intake to psychotomimetic compounds, and from electrophysiology to the anal-

ysis of experience. The speakers represent a variety of theoretical stances, technical skills, and experience.

While each of the presentations is of value for different reasons, it is the one by Elkes which seems to me to place the conference in appropriate focus. Elkes in a short introduction deals with the limitations of the pharmacological approach. There are three levels of attack which pharmacology must follow: the tissue, the animal, and the human. My hearty agreement is secured with the point of view that "the lasting contribution of pharmacology to psychiatry cannot, and will not, depend upon empirical leaps but rather on its steady contribution to our understanding of the brain as an integrating, feeling, information-storing, predicting, and computing organ." Modern pharmacology, as Elkes sees it, is itself inseparable from biochemistry and biophysics. Further, the animal experiment can be but a necessary preliminary in the study of what are uniquely human attributes and functions.

The discussions and the questions of this conference cover a wide range of compounds, of neural, electrical, and chemical effects, of different species as test subjects, and finally of complex social expectations and motivations.

RESEARCH efforts at the frontiers impress me as being like a fugue written by a beginning amateur. There are different, interacting melodies. At times these fit, and at other points there is discord. The discord points in the discussions are always of interest, although these questions and comments do indeed interrupt the flow of the given theme. Sometimes these points become starting points for other, newer workers, or they may reduce to technical trivia. Actually I find the disharmonies and pockets of no-information of value and interest, and I suspect that others do also. Many readers might prefer a different organization and integration of the materials; but, if one is interested in picturing the outlying areas of science, the sparsely settled regions with boundaries ill defined, he must be ready to use a variety of paths.

Such is the picture of scientific effort I have received from each of the Macy

Foundation conferences, and this third conference is no exception. The present volume will take its place among the major sources of background materials in psychopharmacology, and it will be an excellent basis for the integration of the points of view of Beecher, Masserman, Richter; of the technical attacks of Olds, Purpura, and Quastel; and of the biochemical work and thinking of the Brodie-Shore group. The difficulties and problems of bio-assay are well depicted in the discussions by Abramson and Jarvik.

The pharmacological fronts identified by Elkes are being pursued actively and productively, but that is not the whole story. The specific mission of the psychologist to the total problem of the effects of drugs on behavior extends beyond the central theme of this conference. As Elkes put it: "Of environment, forever playing upon the brain detector, and so often disrupting and distorting it, pharmacology will tell us nothing." The psychologist who studies drug effects has a much larger task.

sions of evidence. Over and above commonly used methods of experimentation and testing, we need a far wider use of (a) historical data, (b) data pertaining to "exceptional" people and events, and (c) data pertaining to the variations between experimenters (or interviewers) who collect the facts. As soon as these three extensions of evidence are demanded in the writing of laws, psychology will spread over its subject matter far better than it does now.

On the whole the author's critique is stronger than his proposed solutions. Take his discussion of "exceptional" people. He rightly insists that deviation from the norm is not a mere matter of individual differences, for each life has its own inner coherence and organizes its departures from the norm in its own way. To meet this dilemma of uniqueness the author proposes two different solutions, either of which would be acceptable to him. On the one hand, psychology can be forced to reconstruct its whole approach so that the interrelationship of processes within the single life becomes its dominant concern. Or, on the other hand, it can continue in its present path (if it admits the recommended extensions of evidence) and consign the unique individual to the realm of art. Lafitte does not take sides on this issue since he thinks what comprises a "science" of psychology is a mere semantic matter. Yet it would seem to me that the whole logic of his position requires that he battle to the bitter end for the first of these two alternatives and that he reject the second.

The title of the book is not particularly apt. We find nothing in it pertaining to the theory of personality nor to the philosophy of the person. We find rather a somewhat heavy treatise on methodology presented in the usual slow motion of doctoral theses. Yet for all its heaviness the product has an imaginative sweep. With bold scrutiny it inspects the central paradox of psychological science which in most writings on method is kept darkly under wraps. The author's own background in military and industrial psychology adds weight to his appeal for methods that will make room for "the whole of one's evidence, judgment, experience and insight."

The Science of Whole Lives

Paul Lafitte

The Person in Psychology: Reality or Abstraction? New York: Philosophical Library, 1957. Pp. x + 233. \$6.00.

Reviewed by GORDON W. ALLPORT

Dr. Allport, for more than twenty-five years now, has been a professor of psychology at Harvard University. He is author, as everyone knows, of *Personality: A Psychological Interpretation* (Holt, 1937) and *The Use of Personal Documents in Psychological Science* (Soc. Sci. Res. Council, 1942)—to cite the two of his eight books most closely related to the present review. Lafitte offers two alternatives for counteracting the "positivistic retreat from life," and Allport, of course, makes the only choice you could expect from this militant ideographer.

THE author, a Senior Lecturer at Melbourne, deplores the fact that the current methods of psychology are inadequate to its agenda. The agenda include, first and foremost, he says, the intentional (purposive) behavior of men as found in domestic, school, and occupational settings. At the present time most of our laws are derived from the reaction of subjects to laboratory machines or to mechanical situations. For this reason they have only a remote and analogical bearing on behavior in ordinary life-situations. In brief, contemporary psychology falls short because it is so much smaller than its object.

Lafitte illustrates what he calls "the positivistic retreat from life" by citing Skinner's credo, "I may say that the only differences I expect to see revealed between the behavior of rat and man (aside from enormous differences of complexity) lie in the field of verbal behavior." To this Lafitte replies, "This seems to be a quaint way of saying that man is quite like the white rat except that he is very different in those kinds of behavior that depend on the use or understanding of language. As this includes most human behavior, it would appear that discontinuity is asserted."

The root of the trouble, according to the author, lies in our "loose thinking" when it comes to applying laws of process to persons. It was natural and desirable that psychology as a young science should start by abstracting process (audition, intelligence, the unconscious, etc.) from persons; but it was not desirable for it then to discard persons and to deal as now only with models and metaphors remote from lives as they are lived.

The remedy lies not in returning to pristine common experience (for psychology's job is to "expand, correct, and clarify" common experience). The remedy lies rather in admitting three exten-

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Psychopathology Not Too Experimental

Paul H. Hoch and Joseph Zubin
(Eds.)

Experimental Psychopathology.

(Proceedings of the 45th Annual Meeting of the American Psychopathological Association, New York, June 1955.) New York: Grune & Stratton, 1957. Pp. x + 275. \$6.50.

Reviewed by A. H. BLACK

who, with a fairly recent PhD in psychology at Harvard, under Richard H. Solomon on avoidance learning in dogs and the conditioning of their autonomic responses under curare-like drugs, is now continuing work at Harvard on the experimental analysis of the aversive control of behavior. He is a Research Associate in Harvard's Laboratory of Social Relations.

HERE we have sixteen papers delivered at the 1955 meetings of the American Psychopathological Association. From its title one might have expected a description of recent research on abnormal behavior, but this expectation is not met. The subject matter varies from concern with whether Homer was one man or more, to the conditioning of heart-rate responses in the dog. Five of the papers deal primarily with the conceptual rather than the experimental analysis of pathological processes. Three others present discussions of case histories.

The case-history method, however, can, when applied in a sophisticated manner, provide extremely suggestive data. One such instance, a study being carried out at the New York Hospital Medical Center, is described in this symposium. The subjects, Chinese adults who failed to return to China following the Communist revolution in 1949, were examined by a team of anthropologists,

psychologists, psychiatrists and physicians who tried to record main events in the lives of each subject and their reactions to these events. The data show that the illnesses of each subject seemed to occur at specific periods of their lives separated by long periods free from illness. Further, these periods of illness were correlated with "disruptions of interpersonal relationships" rather than with the occurrence of physical hazards.

THE remaining eight papers of the symposium are devoted to the systematic observation and experimental analysis of behavior. The value of an experimental approach is clearly illustrated in a paper by Brady. He summarizes recent research on factors affecting conditioned emotional responses in the rat. He shows how a careful empirical analysis can illuminate the complex relationships in phenomena which appear simple, even grossly simple, on the surface. As Brady points out, such an empirical analysis makes the identification of conditioned emotional behavior with some "monolithic" state such as anxiety seem premature.

Among the experimental papers, the reviewer found Richter's summary of his research on behavior and metabolic cycles of special interest. Also noteworthy was Lesse's study of the immediate effects of leucotomy on unanesthetized patients. In the process of operating on his subjects, he stimulated the temporal-lobe cortex and was able to find no evidence for the "evoked memories" described by Penfield. Finally, the reviewer was dismayed by the failure of some of the experimenters, who studied the effects of drugs on human behavior, to use control groups and placebos (not to mention double-blind controls). The results of such uncontrolled experiments are always open to question.

It is extremely difficult to evaluate any symposium as a whole. Many of the papers in this book are clearly well worth reading. Nevertheless, if the word *experimental* in the title of the book is an indication of the editors' aims, then I do not feel that they have been successful.

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ADOLPH MANOIL
Film Editor

Film Review

Mental Deficiency

Clinical Types of Mental Deficiency (formerly *The Feeble-minded*)

Produced by Audio-Visual Education Service, University of Minnesota in cooperation with Faribault State School and Hospital and Cambridge State School and Hospital. 16-mm. black and white, sound, 39 min., 1957. Available through Psychological Cinema Register. \$175.00, rental \$6.50, and other sources.

Reviewed by GEORGE M. GUTHRIE

The Pennsylvania State University

FOR many years *The Feeble-minded* has been about the only film available on mental deficiency. It was unsatisfactory for classroom use because the cases presented were so severe that all except most advanced students were usually upset. For example, the case of grand mal epilepsy was of the patient who held the record in the hospital for the largest number of seizures. The clinical material from the old film has now been retained in the new. The concluding argument for eugenic sterilization has been eliminated. A new introduction has been added showing some scenes about the institution which give some idea of the institution program.

With this introduction, the new film retains the old and very useful demonstration of the performances of morons, imbeciles, and idiots on the Manikin and Seguin Form Board tests. Differences in ability are quite clear from this performance, but, since they are beyond the purpose of the film, the implications of these differences for training and care are not spelled out. The major portion of the film is taken up with a sequence in which cases of the rare oxycephalus

and hypertelorism and the more frequent microcephalus, hydrocephalus, cretinism, and mongolism are literally dragged before the camera. Finally, although these types are not always mentally deficient—the film makes this clear—cases of epilepsy and cerebral palsy are also shown. The material presented in this film will substitute for a trip to the back wards of an institution for mental defectives.

Such a trip, however, gives a very distorted picture of the problem. A film is badly needed to show the differences and similarities of the vast majority of the retarded, a film that indicates the possibilities and limits of training. Films are also needed to demonstrate the differences in social effectiveness of persons of similar IQ. In these days of emphasis on the retarded child, the possibilities of public education by means of films seem to have been largely ignored. Since it brings together in a short period of time extreme and unusual cases, the film now under review will be most useful for an advanced seminar in mental deficiency or in disorders of the central nervous system. The film has also been found to enable students to tolerate better the ordeal of a field trip to a school for the retarded.

Filmstrip

Perception

Is Seeing Always Believing?

Herma B. Hudson, Washington Junior High School, Racine, Wisconsin. Available through Visual Education Consultants, Inc., Madison 4, Wisconsin. \$3.50.

Reviewed by HARRY C. MAHAN

Palomar College, San Marcos, California

This filmstrip, although presumably produced for junior high-school pupils,

can be used to considerable advantage in courses in general psychology. It consists of a series of seventeen ambiguous figures and illusions similar to those frequently found in textbooks of general psychology. Beginning with the parallel-gram illusions, it progresses through illusions of size, perspective, parallel columns, lines, and space. The last six frames illustrate various ambiguous figures.

The strip is well designed and, although it includes captions and a script, the user will no doubt wish to carry on his own discussion during its presentation. This reviewer found that it elicited considerable interest from the students, one of whom operated the projector while the instructor stood beside the screen with a pointer discussing each figure as it was presented.

The time taken for the entire strip was from 30 to 40 min.

The strip is, however, too short. There are many other interesting figures which might have been included. Nevertheless it is well worth its nominal cost and perhaps a sequel may eventually be made available.

Films and Other Materials

Book

James D. Finn

The Audio-Visual Equipment Manual. New York: The Dryden Press, 1957. Pp. xx + 363. \$9.50.

Film

MENTAL HEALTH

The Key Produced for the National Association for Mental Health by Campus Film Productions. 16-mm black and white, sound, approx. 32 min., 1957. Available through National Association for Mental Health Film Library, 13 E. 37th St., New York 16, N. Y., and Contemporary Films, 13 E. 37th St., New York 16, N. Y. \$145.00, rental \$7.00.

The film presents the problem of mental illness in its social and individual aspects. The need for public awareness of the problems of mental health is stressed.

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